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Equation of the Moon, and closes his note with the following words:—"It would be a point of interest to determine whether Professor Hansen's theory would bear any considerable increase in the mean Solar Parallax." I fulfil this desire with all the more pleasure, since the determination of the Solar Parallax from my Lunar Tables may be obtained by means of data which have been already published.

On page 177 of the *Darlegung der theoretischen Berechnung der in den Mondtafeln angewandten Störungen*, it will be found, that in my calculation of perturbations for the ratio of the major semi-axis of the orbits of the Earth and Moon, I have adopted the following value:—

$$\log \frac{a}{a'} = 74034757 \cdot$$

which gives, for the co-efficient of the Parallactic Equation 121″·368 (Lunar Tables, p. 8). By comparison with observations, however, it results that all inequalities, theoretically calculated with the above value of $\frac{a}{a'}$, must be multiplied by the factor 1·03573 in order to conform to the observations. When, therefore, the above-mentioned co-efficient is multiplied by this factor, it gives 125″·70, which coincides very nearly with the value which Mr. Airy has obtained by the Altazimuth.

Further, in the Lunar Tables (p. 4), the constant of the Parallax of the Moon is given as = 56′ 59″·57. This being multiplied by the above value $\frac{a}{a'}$, we have 8″·66 for the Solar parallax, which has been adopted as the basis of the theoretical calculations. This, again, being multiplied by the above factor 1·03573, the Solar Parallax, determined by the elaboration of the Lunar Tables, becomes 8″·97. This value coincides very nearly with that which has been determined by the last opposition of *Mars*.

On the Companions of Sirius. By M. Goldschmidt.

(*Extract from a Letter to Admiral Manners, Secretary of the Royal Astronomical Society.*)

I have seen with satisfaction the insertion of my letter from M. l'Abbé Moigno's journal, *Les Mondes*, in your *Monthly Notices*, No. 5, vol. xxiii. I hope that *Sirius* will be observed next autumn by astronomers provided with large telescopes, to verify the minute stars I saw in its following vicinity. As long as *Sirius* was high enough above the horizon, I could see them; but afterwards I perceived only mere glimpses of light,

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without being able to distinguish the position of the stars. On the top of the wood-cut in the *Monthly Notices*, No. 5, is written "*Position des Satellites:*" these words are not mine, for I cannot believe that they are all satellites. In the possibility that some of these luminous points could perhaps be the result of optical illusion or sickness of my eyes, I tried the same kind of observation (by passages) with some brilliant stars, like *Altair* and *Antares*, but I could see nothing like that I perceived on *Sirius*; and so, having done all that is in my power, with an object-glass of only 4 inches (English measure) or 46 lines, to snatch such minute light points, I leave it to more skilful astronomers to do the rest; for a powerful telescope with clock-work and the star hidden behind a large plate would show this wondrous system. The distance of the star D I tried to measure on the moment of its discovery, and it might be distant east from *Sirius* $3^s\cdot60$ or $3^s\cdot80$ in Right Ascension. I found also another fainter and distant star (perhaps $10''$) in the vicinity, and on the east of the star D, which I announced in my first letter to Mr. Peters, on the 8th of March. On writing this, I received the news of Rev. Mr. Dawes' observation of the star D. It is, indeed, the most conspicuous of all by its *distant position*; but I think that the two nearest ones are more brilliant, and seem to be bodies of great importance relative to *Sirius*.

Paris, le 5 Juin, 1863.

On the Discordance between the Results for Zenith Distances obtained by direct Observation and those obtained by Observation by Reflexion from the Surface of Quicksilver. By G. B. Airy, Esq., Astronomer Royal. (Abstract.)

After explaining the manner in which the discordance in question is practically taken account of at the Greenwich Observatory, the author remarks that, in preparing for the formation of the new Seven-Year Greenwich Catalogue of Stars, it appeared to him desirable to examine carefully the results of the reductions (extending over a quarter of a century) which have been conducted on the principle described, with the view of ascertaining whether the Direct Observations ought to be used alone, or whether the Reflexion Observations ought to be used alone, or whether the mean of them ought to be used, as has been done in the Annual Reductions; and that the object of the Memoir is to give the result of this inquiry. It appears that neither the D observations nor the R observations can be adopted, without correction, as sufficiently accurate, but that the D observations corrected by $\frac{1}{2}$ (R — D), or still better by $\frac{3}{5}$ (R — D), give accordant values

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